Verhoeff (F. H.)

A Description of the Reflecting Phorometer and a Discussion of the Possibilities Concerning Torsion of the Eyes.

~~Presented~~

By FREDERICK HERMAN VERHOEFF, Ph B., M.D.,

BALTIMORE, MD.

[*Reprinted from American Ophthalmological Transactions, 1899.*]

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A DESCRIPTION OF THE REFLECTING PHOROMETER AND A DISCUSSION OF THE POSSIBILITIES CONCERNING TORSION OF THE EYES.*

By FREDERICK HERMAN VERHOEFF, Ph B., M.D.,

BALTIMORE, MD.

Through the kindness of Dr. Theobald I have been invited to describe to-day a new phorometer that I devised during the past year. A description of my original model may be found in the Johns Hopkins Hospital Bulletin for May, 1899. Some time ago I placed my model in the hands of E. B. Meyrowitz of New York, who, under my supervision, has made some changes in the mechanical details of the instrument, but the general principles remain the same. I may say that the original model has been in practical use at the Johns Hopkins Hospital Dispensary and also at the Baltimore Eye, Ear, and Throat Charity Hospital, and has proved satisfactory in every way.

The instrument, Fig. 1, differs from other phorometers in that mirrors are used in place of prisms, thus permitting of the greatest simplicity in construction together with the most advantageous movements in the apparent images. The only objection to the mirrors is that each of them produces a double image of the object, due to reflection from the surface of the glass. Where a white spot upon a black background † is used the reflection from the surface of the glass is so faint in comparison with the image from the silvered surface that it is hardly noticeable. As a matter of fact, I have never known it to cause the slightest confusion to the patient, it being unnoticed by the latter. By making the mirrors of speculum metal or by using silvered prisms this double reflection could be entirely overcome, but either method would entail a needless expense.

The arrangement of the mirrors is shown in the diagrams, Figs. 2 and 3. In the new model, however, the mirrors are

* Read by invitation of the society at the 35th annual meeting.

† A black spot upon a white background is still better. It is only when a very bright object, such as a lamp, is used that the double reflections become evident, and even then they cause no confusion.

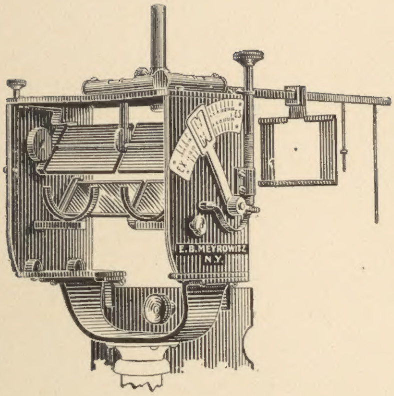

FIG. I.

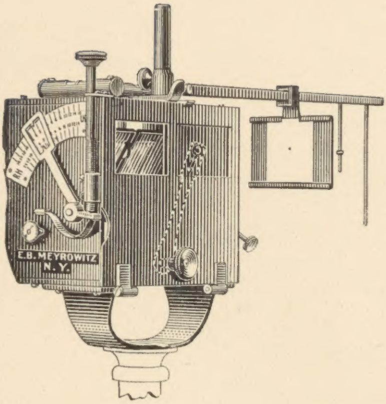

FIG. I.

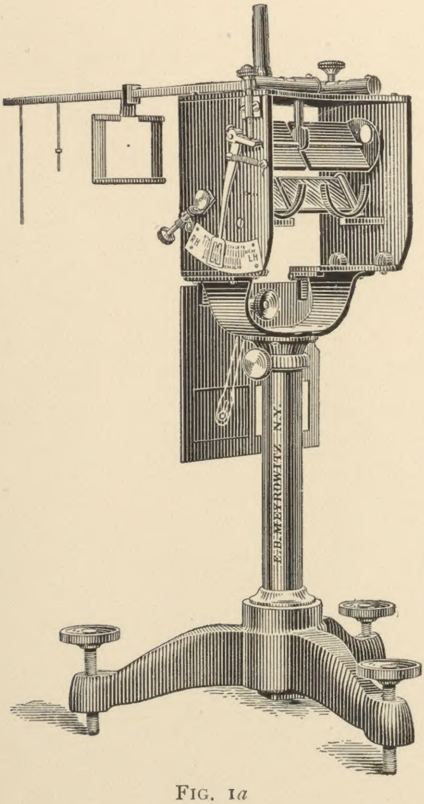

FIG. I*a*

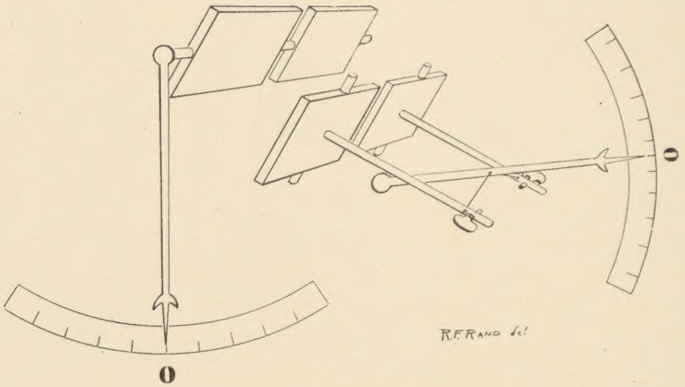

FIG. 2.

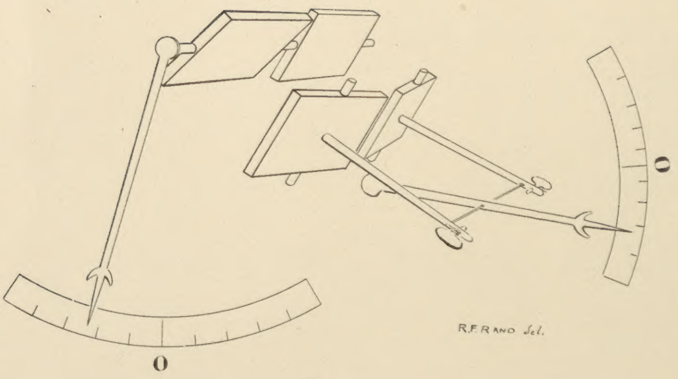

FIG 3

moved by eccentrics and not directly by levers. The axes of the two upper mirrors are in the same line and are parallel to the horizon and perpendicular to the direction of sight, while the axes of the lower mirrors are parallel to each other and lie in planes perpendicular to the horizon and parallel to the direction of sight. The inclination of the mirrors is about 36° to the perpendicular. By turning a milled headed screw the lower mirrors may be made to rotate towards or away from each other, lateral separation of the images thus being produced. It is to be noted that this arrangement produces equal, though opposite, movements in the two images. Vertical separation of the images is produced by rotation of one of the upper mirrors, this being accomplished also by means of a milled headed screw.

I shall not discuss here the method of graduating the scales of the instrument. Anyone who is interested will find it sufficiently explained in the article referred to. Two sets of scales are provided, one for distant points — 4.5 to 6 meters — and one for the near point — 30 cm.*

Clips are provided at the back of the instrument so that auxiliary prisms may be used when desired. A double level is attached to the instrument and it is important that the latter should be kept level. It is also important to have the object directly in front of and at about the same height as the instrument.

The diagram, Fig. 4, is intended to illustrate in as simple a manner as possible the construction involved in locating the position of the image for each eye. The relations of object and mirrors to each other are exaggerated in order that the construction lines may be more plainly seen. The mirrors are in their primary positions, so that a line drawn perpendicular to the axes of the upper and lower mirrors will be perpendicular to the plane of the lower mirror. O is the object, M and M^1 the mirrors. From O, a line O A is so drawn that it is perpendicular to and bisected by the prolongation of M. Similarly from A the line A I is drawn perpendicular to and bisected by the prolongation of M^1 at K. All the rays from O striking M will be

*The scales are now graduated by means of one standard prism.

reflected in lines directed from A, and these rays will be reflected from M^1, in lines directed from I. Therefore, an eye directed towards the mirror M^1, will see the image of O at I. The actual path taken by a ray of light from O is indicated by the line O B P E. The locus of A, as M is rotated on its axis R, is evidently the circumference of a circle whose radius is O R. The locus of I during this rotation is the circumference of a circle with the same radius but whose center is at C, R C being drawn perpendicular to, and being bisected by, the prolongation of M^1.

When M^1 is rotated on its axis, which lies in the plane of the construction, it is evident that I will move along the circumference of a circle perpendicular to the prolongation of M^1 and whose center is K. This circle being at an angle to the line of sight, I will apparently take an elliptical path.

From this it will be seen that when the lower mirrors are moved the image pertaining to each moves in the circumference of a circle tilted at an angle of 36 degrees to the perpendicular, and whose center is at the foot of the perpendicular drawn from the object to the axis of the mirror. Since the projection of a circle is an ellipse, the image of each mirror will apparently move in an elliptical course, and will thus not only move laterally, but also upwards to a slight extent. This, of course, would seriously interfere with the accuracy of the instrument if each lower mirror were rotated independently, but, by the arrangement previously described, both mirrors are made to move equally though in opposite directions, and hence the images when viewed with both eyes maintain their horizontality. If the test object is a perpendicular line its image will generate the surface of a cone, and thus when projected the two images, as they are carried apart, will make increasing angles with each other.

A very important feature of this phorometer is a shutter which slides back and forth before the eyes, so that when it is in use the patient can see with but one eye at a time. The shutter is moved back and forth very quickly, a pause being made, however, before each eye, so that time is given for the after image to disappear, and the patient is asked to state whether the object moves or not. If he sees the object apparently moving obliquely, the upper

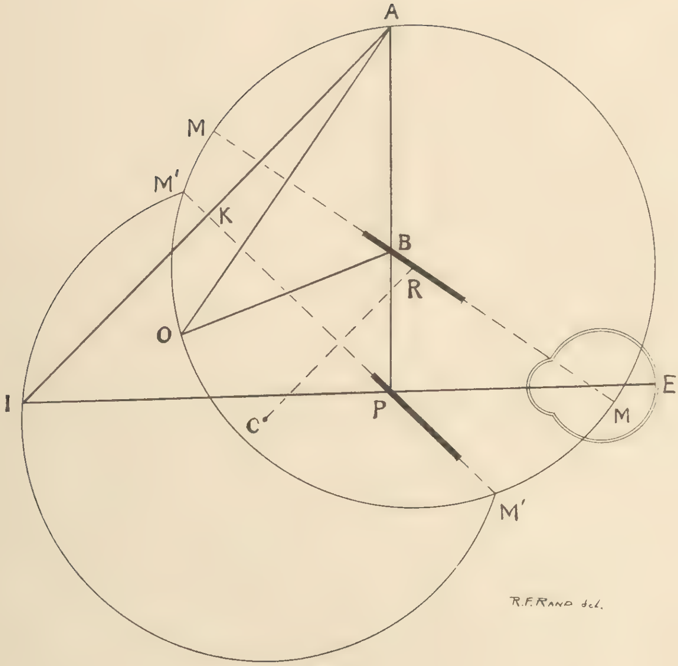

FIG. 4.

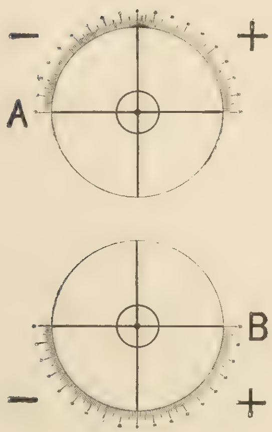

FIG. 5.

mirror is adjusted until the movement is horizontal, and then the lower mirrors are adjusted until practically all movement is overcome. Both the amount of hyperphoria and also the esophoria or exophoria, if present, will be indicated upon the scales.

Since adapting this test to the phorometer, I have found that in principle it is the same as the parallax test of Duane.* It has, however, so far as I know, never hitherto been adapted to a phorometer, and I do not believe that the accuracy of this test has been sufficiently recognized.†

Anyone who has tried the diplopia test upon himself must have been struck by the oscillating movements of the images. This is, no doubt, due to the fact that so long as there are images upon both retinas there will be a stimulus to convergence and perhaps other conflicting stimuli. This is supported by the fact that the eyes under these circumstances always give the sensation of being more or less under a strain. This difficulty does not enter into the shutter test. Again, in order to produce diplopia it is necessary to separate the images so great a distance that it is not only somewhat difficult for the patient to judge accurately of their relative positions, but, with the trial prisms at least, one is never sure but that the lack of horizontality may be due to the method of producing the diplopia, it being necessary not only to keep the patient's head level, a difficult undertaking both literally and figuratively speaking, but to have the bases of the prisms very accurately placed. These difficulties are most marked in testing for hyperphoria, a condition in which accuracy is most demanded. By means of the shutter test, however, both images are finally located exactly upon the two foveas, the ocular muscles at the same time being entirely at ease.

At a matter of fact, I have found that in apparently normal eyes where there is orthophoria at 6 meters (20 ft.)‡ by the diplopia test, in almost all instances the shutter test shows an exophoria of about 2°, and in cases of esophoria the shutter has demonstrated

* *New York Medical Journal*, August 3, 1889.

† In high grades of lateral insufficiency this list may be made by observing the movements of the eyes of the patient as the shutter moves back and forth, and then adjusting the mirrors until the eyes remain stationary. In some cases this is the only test that will avail.

‡ For an interocular distance of 6.6 cm., it requires a convergence of about 1¼° to fix a point 6 meters distant, and hence orthophoria at this distance would mean an actual esophoria of 1¼°.

about 2° less than the diplopia test. As to hyperphoria, in my own case, for instance, I have easily detected ¾° of left hyperphoria which was impossible for me to detect by the diplopia test. Duane, I believe, also found the test especially valuable in cases of hyperphoria.

Stevens' phorometer possesses all the disadvantages incident to the diplopia test, to which test its usefulness is limited, and it is impossible to adapt the shutter test to this instrument. On the other hand, with the reflecting phorometer the diplopia test may be made with equal facility and with the advantage that the amount of separation of the images may be regulated at will. In addition to this, by means of the reflecting phorometer the abduction, adduction, and right or left sursumduction of the eyes may be readily obtained and the instrument may also be used for exercising the eye muscles.

The reflecting phorometer, in connection with a special chart, may be used to estimate the amount of rotation of the eyes about their optic axes. The chart to be used in practice consists simply of two rotary disks placed one above the other upon a flat background, Fig. 5. Degrees are marked out upon the board so that either disk may be rotated any number of degrees desired. Various figures are drawn upon the disks and they, having been placed at a distance of 6 meters in front of the phorometer, are superimposed by means of the hyperphoria screw.

In practice, the best figures for measuring torsion are two right angles such as Stevens sometimes uses in his clinoscope. These are so placed that when combined by a normal eye the appearance of a rectangular cross is produced. In my original article, I stated that torsional combining power could be accurately measured by means of two rectangular crosses. I should have said that this was only theoretically true, for I immediately proceeded to show that as a matter of fact torsional combining power could not be accurately measured in this way.

Theoretically, with a flat board, both disks could not be placed perpendicular to the line of sight. Practically, it is impossible to recognize the excessively slight error this produces. It may be overcome, if desired, by having the board consist of two pieces

hinged together so that they may be set at any angle. I do not find, however, that this is at all superior to the flat board.

The visual plane may be varied either by changing the position of the chart or by altering the inclination of the face. The latter, being the natural method, is the better. The visual plane may also be varied to a certain extent by changing the inclination of both upper mirrors. If it is desired to shut out all other images, tubes may be used. This is in reality unnecessary.

The superposition of the disks could be produced for practical purposes by other means, a Risley prism mounted in a fixed frame, for instance, but the reflecting phorometer offers a most convenient and accurate means of doing this.

In many ways Stevens' clinoscope is a valuable instrument, but it possesses no real advantage over the reflecting phorometer. In fact, it possesses the following disadvantages: (1) Since the tubes are only ½ meter in length, it is necessary for emmetropic eyes to be aided by a + 2D spherical lens in order to preserve the normal relation of convergence and accommodation; (2) an adjustment for the interocular distance is necessary, and as accomplished in the clinoscope this produces a lateral tilting of the disks; (3) it is difficult to hold the disks in union; (4) it is necessary that the patient keep his head perfectly level; (5) it is practically impossible (so far as the surgeon is concerned) to substitute other disks for those attached to the instrument, and the size of disks and the distance at which they are viewed cannot be varied.

DISCUSSION OF THE POSSIBILITIES CONCERNING TORSION OF THE EYES.

As regards torsion of the eyes, there are four conceivable possibilities as follows:

(1) Non-neutralizing declinations* of the two eyes. These include declinations which are numerically equal, but which have the same sign.

* The useful term "declination" was introduced by Stevens to designate that condition in which the apparent vertical meridian of one eye is at an angle to the real vertical meridian, it being understood that all of the retinal meridians have undergone similar displacement. Outward declinations are positive, inward declinations negative in sign.

(2) Neutralizing declinations of the two eyes. These are numerically equal, but have opposite signs.

(3) Torsional combining power or an ability so to rotate as to overcome artificially produced declinations.

(4) Cyclophoria, a tendency for the eyes to undergo torsion, which, however, is overcome by the action of certain of the ocular muscles.

It is obvious that (2) could cause no difficulty in vision. A better definition for (1) would be, a condition in which the apparent vertical meridians are never parallel, and for this condition I have suggested that the term torsional strabismus or squint be used. If it can be shown that (3) has no existence, it follows immediately that cyclophoria, (4),* must be simply an imaginary condition also, (1) then being the only remaining possibility of any moment. I shall now proceed to show that, as a matter of fact, the eyes do not possess a torsional combining power, (3).

In the paper to which reference has already been made, I reported three experiments which abundantly proved the non-existence of a torsional combining power. These experiments were selected from among many others as being very simple, as well as conclusive. I shall briefly repeat these here in a modified form, and, in addition, describe another experiment which shows not only that lines making an angle with each other upon the two retinas are not fused by a process of torsion, but that absolutely no torsion occurs in the eyes even inadvertently during the experiment.

Experiment I. (Fig. 6.) By means of the hyperphoria screw of the reflecting phorometer, the image of disk A, which has been rotated 8°, is superimposed upon the image of disk B. Disk C represents the appearance then presented to the eyes. It is seen that the horizontal line appears double, while the vertical line is single. It is obvious that if the two vertical lines were com-

* F. B. Eaton (*Journal of American Medical Association*, September, 1894), has summed up evidence to show that the ordinary tests for cyclophoria are misleading. He does not offer conclusive proof, however, that cyclophoria is an impossibility. He does not discuss the possibility of torsional squint, nor, in general, of torsional combining power; but he shows the absurdity of supposing that the distortion produced in retinal images by astigmatism could be overcome by torsion of the eyes.

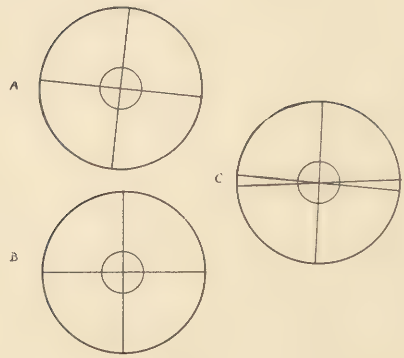

Fig. 6.

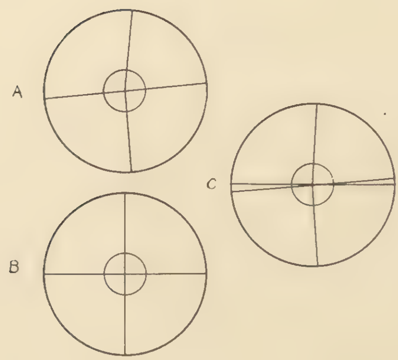

Fig. 7.

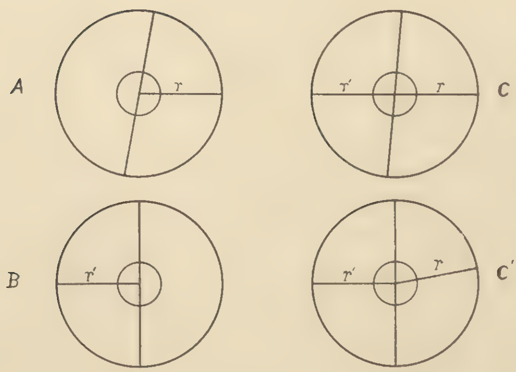

Fig. 8.

bined by torsion of the eyes, the horizontal lines would of necessity also be combined.

Experiment II. Using the same disks as in *Exp. I*, the maximum amount of separation of the vertical lines at which they still remain fused is determined. The same determination is then made, using much larger disks. A much smaller separation is allowed in the latter case. In my case, with disks 3 cm. in diameter, viewed at a distance of 3 meters, the vertical lines remain single when there is a separation of 18°-20°; but when disks 12¼ cm. in diameter are viewed at the same distance, a separation of only 8° is allowed. Torsion could not explain this.

Experiment III. (Fig. 7.) The upper and lower parts of the vertical on A have each been placed at an angle of 5 degrees to the perpendicular, and are thus at an angle of 10 degrees to each other. The horizontal line remains perpendicular to the lower part of the vertical line, and hence has been rotated 5 degrees. C shows the appearance when A and B are superimposed. It will be noticed that the angle between the two parts of the displaced vertical line on A is less than the corresponding angle on C. It is inconceivable how torsion could produce this result.

Experiment IV. (Fig. 8.) When A and B are superimposed it will be noticed that although the displaced vertical line on A is fused with the vertical line on B., the two radii r and r^1 remain horizontal. If torsion occurred these two radii would of necessity appear at an angle to each other as figured in C^1. If disk A be rotated very slightly so that the radius r is no longer horizontal it will be almost immediately noticed in C, thus showing that if torsion should occur the radii would plainly show it.

I have made many other similar experiments, all of which prove the same thing, *i. e.*, the non-existence of a torsional combining power.

Wheatstone,* the inventor of the stereoscope, realized that the images of lines which were placed at an angle to each other upon the two retinas, or, more generally speaking, lines which

* Phil. Trans., 1838; also Phil. Mag., 1852.

2

occupied non-corresponding points, could still be fused. This fact led him to repudiate the law of corresponding points. The possibility of the phenomenon being due to torsion did not occur to him, and he therefore did not undertake to disprove this hypothesis.

Since Wheatstone's original paper there has been much written upon this subject, and the general opinion seems to be that the law of corresponding points is, within physiological limits, rigorously true. It has been stated that there is no actual fusion of the lines in stereoscopic pictures or in binocular vision in general, and that if the attention be directed towards it the doubling can be detected in every instance.* This view, however, I believe to be erroneous, for my experiments have shown that ordinarily there is in reality no such doubling, but that a single line is produced by the fusion of two others, and that, moreover, this single line is *intermediate*† in position with regard to the other two.

I admit, however, that with practice it is possible to detect the doubling of lines when they are displaced at a distance at which formerly they were completely fused, but when this is done the stereoscopic effect is either lost or much impaired. This upholds the law of corresponding points, but in no way disproves the view of Wheatstone that stereoscopic relief is due to *fusion* of lines that are situated upon non-corresponding points.

Since this fusion is not produced by torsion of the eyes, it must be a psychical phenomenon, and I have spoken of it as being due to a psychical compensation. As shown in experiment (1), this psychical compensation is more marked for lines which have been displaced from the vertical than those displaced from the horizontal. This is not strange when we consider that in viewing perspectives with both eyes, the head being erect, there is no doubling of horizontal lines to be overcome.

It is an interesting fact that the psychical compensation is increased by muscle strain. Stevens makes a similar statement

* Brewster, Phil. Mag., 1844, p. 363. Le Conte, "Sight," 1897, p. 154, 170, 174.

† This proves that the phenomenon is not due to a suppression of the image of one of the lines.

when he says,* "As to the extent to which torsion can be carried, it is greater in convergence than in parallelism of the eyes." Another interesting feature of this psychical compensation is that it renders it possible to fuse a straight line with a curved one. This gives an effect of perspective to the resulting curve. Torsion evidently could not account for this.

A discussion as to the possible nervous mechanism by which the psychical compensation is accomplished, and also further considerations concerning stereoscopic vision, would lead too far.

The existence of a torsional combining power has been assumed by Stevens without sufficient evidence, and from this erroneous assumption, he has drawn far-reaching conclusions. For instance, he says,† "To such a person, with the head in a position nearly upright, the images of horizontal lines seen by the two eyes would be inclined to cross and thus cause confusion. To avoid such confusion the head must be thrown far forward or an irksome voluntary effort directed to the torsional muscles must be brought to bear."

Again he says, "The muscles which principally influence torsion are, notwithstanding their comparative size, less able to exercise constant exertion than those which act in a more direct manner upon the vertical and lateral movements. Hence, a position of the horopter demanding considerable torsion through the influence of the oblique muscles is a position resulting in fatigue and often in pain and exhaustion."

Referring to torsion artificially produced, "That the oblique muscles play the principal role in the act of torsion, under circumstances such as have been mentioned, is unquestioned. They are not, however, the only agents in the rotary movements, for the superior and inferior recti lend important aid in the act."

Further, he says, "With a good power of torsion, the lines may be held in union until each of the pointers mark + 11°. Thus, between the vertical retinal meridians of the two eyes an angle of 22° with its apex down is formed." In the charts that

* Arch. of Oph., vol. xxvi, 1897, p. 199.

† Op. cit., p. 195.

I have recommended, the disks are so large that I, at least, can prove a psychical compensation of only 8°. With smaller disks, however, I have also obtained perfect fusion when the lines were separated 22°. A torsional movement of 22° in one eye, or even of 11° in each eye, could undoubtedly be seen by an on-looker. Stevens, however, says nothing with regard to this point.

If it be the duty of the oblique muscles to keep the apparent vertical meridians of the retina upright, they are certainly very negligent. Otherwise, why do they not overcome the torsion which, as is well known, is produced in the eyes during their various excursions, during convergence with elevation of the visual plane, for instance? The truth of the matter is, that such torsion is practically never so great as to be beyond the limits of psychical compensation.

"The fact of difference in torsion for horizontal and vertical lines has its practical application in many directions." What Stevens had really noticed was the difference in the psychical compensation for the two meridians of which I have already spoken. He apparently did not realize that this could be made evident in one experiment. If the term, psychical compensation be substituted for torsion, Stevens' statement, just quoted, is probably correct.

Stevens claims that exophoria and esophoria are due to the efforts of the oblique muscles in overcoming torsion, and carries this view so far that he says: . . . "I am sure that I shall make no mistake in saying that when the excesses of vertical rotation of the eyes are considered in connection with the normal declinations of the retinal meridians, it will be no longer necessary to perform the well-known and standard operations for converging or diverging squint."* It seems to me that it is much more likely that torsion is the *result* of exophoria or esophoria than that the reverse is the case.

The fact that when in the reflecting phorometer lateral separation of the images is produced, there also occurs lateral inclination of these images, instead of being a disadvantage is quite

* Annals of Oph., April, 1899, p. 162.

the reverse. Just as prisms can be used to reproduce the conditions of squint, so this instrument can be used to reproduce the effect of torsion and in this way the lateral separation of the images being overcome by means of prisms, a little over 8° of apparent torsion can be obtained.

I find that when I carefully avoid artificial hyperphoria I can view without discomfort a scene containing houses, even when there is an artificial torsion of 8°. A torsion greater than this produces a disagreeable doubling of horizontal lines. A doubling which does not occur within the limits of distinct vision is not at all disagreeable. In no case have I noticed any special strain upon my eyes during these experiments, but this may be due to the fact that I have become accustomed to such experiments. If thought desirable, the property of the instrument just mentioned could be much increased by altering the inclination of the mirrors. This, however, would render the instrument unsuitable as an ordinary phorometer.

The phorometer does not, in fact, reproduce the *exact* conditions which would be present in torsional squint, for the eyes must constantly be directed at the fixed point of crossing of the lines in the two images of the view, whereas, in torsional squint the crossing point always corresponds to the point of fixation of the eyes and hence follows, as it were, their movements. If the instrument be moved in unison with the eyes, this difficulty is overcome.

It is not unlikely that in cases of torsional squint a higher grade of psychical compensation may be developed for horizontal lines than is normally the case.

In this connection it might be well to speak of a feature of the shutter test which I neglected to mention in my first article, and that is, its power to make evident the slightest amount of torsion, either real or artificial. When it is desired to determine the amount of torsion in this way the heterophoria of the patient must be accurately corrected by means of prisms and a straight line must be used as an object. If, then, the shutter be moved the line will apparently swing from side to side.

This test has convinced me that my own eyes, as regards torsion, are very slightly out of true. My apparent vertical meridians are tilted in the same direction as were those of Helmholtz, that is, outwards, so that a straight line by the shutter test seems to rotate inwards. In my case, however, the horizontal meridians are tilted to an equal extent. The angle between my apparent vertical meridians is about 1°, while in the case of Helmholtz it was 2½°.

When pure artificial torsion is produced this shutter test gives a decided tilting to a straight line or to any of the objects regarded, thus confirming, what I have already proved, that the eyes do not overcome torsion in objects by undergoing torsion themselves.

If, then, the eyes do not possess a torsional combining power, it follows, as I have already stated, that the term cyclophoria must express a purely imaginary condition, and of this I feel convinced. It also follows that torsional squint is, practically speaking, the only torsional deviation of the eyes to be studied. It is well known that this latter condition occurs where there is a paralysis of certain ocular muscles, but here, of course, one does not get pure torsion. From personal observation, I am unprepared to say whether torsional squint of considerable degree, and unaccompanied by any other form of squint, exists or not, but the observations of Stevens go to show that it is not an uncommon condition. That it alone often causes asthenopic symptoms, I very much doubt, however, for, as I have already stated, I have been able to view a scene without discomfort in which there was a total artificial torsion of 8°. This is not surprising when we consider that in viewing binocular perspectives, there is often an unavoidable lateral angular displacement of retinal images of even more than 8°, to which everyone must become accustomed. Stevens* recognized this latter fact and went so far as to assume that in stereoscopic vision this separation is overcome by torsion of the eyes, a view entirely unsupported by facts and which my experiments completely disprove.

* Arch. of Oph., 1897, p. 202.

It seems to me that instead of the oblique muscles suffering from strain in so far as they are connected with torsion, the recti, if any, would be the ones to suffer. This is so because it would be necessary for the recti to run the point of sight back and forth more than in the case of normal eyes.

It is evident that since psychical compensation is least marked with regard to horizontal lines, strictly speaking, investigations should be made with regard to these lines rather than with regard to vertical ones.

G. C. Savage * believes so strongly in cyclophoria that he has prescribed a —.50D cylinder for the sole purpose of correcting it. Even if cyclophoria existed, such a cylinder would simply make matters worse, for while it might true, for instance, vertical lines, it would throw horizontal lines more out of true than ever. Anyone can convince himself of this by viewing a right angle through a concave cylinder with its axis placed half way between the sides of this right angle. Instead of the latter being tilted as a whole, it is converted into an acute angle.†

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* Oph. Record, January, 1899, p. 34.

† This point was brought out by F. B. Eaton, Op. cit.